

Technology Development

Background:

Currently, Heliophysics technology development is “bottom-up”, with a relatively modest investment (~\$15M per year in H-TIDeS, plus one quarter of the \$5M/year SMD-wide Cubesat program). Technology development is community-driven, and while it is informed by the Decadal Survey and Roadmap, is not strictly tied to those goals, or to particular long-term goals to prepare Heliophysics for the next generation of missions.

In the H-TIDeS program, ~4-6 instrument development proposals are selected per year (including solar, heliosphere, magnetosphere, and ITM), for three years at ~\$150k/year, and approximately 5-7 suborbital investigations are selected per year (including Cubesats, sounding rockets, ISS payloads, and balloons, across all four subdivisions). The H-TIDeS IDP program tends to fund concepts that are in the TRL 4-6 stage, with little support for more speculative but potentially rewarding ideas. Finally, there is a “valley of death” for instrument concepts that may need significant investments (above \$500k) to develop, or that need access to space for longer than a fifteen minute sounding rocket flight, or larger volume than can be accommodated in a Cubesat, and for which the ISS is not a suitable platform.

Heliophysics has also, in the past, made use of the “Category 3” rating for Explorer and Mission of Opportunity proposals, in which a mission concept is deemed scientifically compelling but technologically immature. When a proposal receives this rating, it is possible for HPD to provide technology development funding, typically at a higher level than H-TIDeS, to permit the proposers to further develop their concept for the next competition. This “Category 3” rating has not been used frequently, however, making it difficult to develop more expensive technologies that do not fit into an H-TIDeS envelope.

Heliophysics does have access to other NASA-wide technology development programs, including STMD programs such as NIAC, as well as SBIR. In principle, these can help to develop cross-cutting technologies that may be of interest to Heliophysics missions, but are less likely to fund things like instrument development.

In contrast, other NASA Science Divisions have proportionally larger investments in technology development, particularly Earth Science, with the Earth Science Technology Office (ESTO). This is a ~\$60M effort that provides funding to develop advanced technology in support of Earth Science needs. Earth Science also has a significant Earth Venture program, with opportunities offered every 18 months, and with a wide range of cost caps and platforms, to provide spaceflight testing of new instruments. Planetary and Astrophysics also have multiple technology development programs, including programs to specifically develop TRL 1-3 concepts.

Finding:

Using the rebalanced funding profile available as part of the DRIVE initiative, we anticipate that the number of H-TIDeS IDP and LCAS selections will increase from 4-6 / year to approximately 6-10 (roughly evenly divided between subdisciplines). Even with this positive development, there may be advantages in investing in several other technology investment areas:

- 1) Applying some of the anticipated increase in funding for the Explorer line to the more frequent selection of suitable Category 3 proposals would benefit technology development within heliophysics. Funding on the order of \$1-2M from the Explorer line for one or two proposals could help overcome the “valley of death”. The most benefit will be derived if the announcement of opportunity specifies the approximate number of proposals and the amount of funding that can be invested in Category 3 proposals, so that the community knows that proposals for more risky but important technologies may be rewarded.
- 2) The Science Definition Teams for STP and LWS missions, or other advisory groups that help to formulate missions, could assist the HPD by identifying technology developments that may be needed for high priority science targets. These technology development “roadmaps” could then inform H-TIDeS prioritization and selection. Or, it may be desirable for the STP or LWS lines to invest further additional funds to bring these technologies to readiness before Phase A begins. Currently, these developments are either taken on pre-Phase A by institutions with “deep pockets”, or during Phase A, adding cost and schedule risk to strategic missions. By developing these technologies before the AO is even released, the cost estimates for these missions could be made much more reliable.
- 3) It may be advantageous to increase emphasis on developing long-term technology needs, getting ready for the next Decadal Survey.
- 4) It may be beneficial if a mechanism can be developed by which IDP and other developed instrument concepts can gain access to space. This could include HPD-funded hosted payloads on the ISS, other vehicles, and on sounding rockets, etc. Potentially NASA could consider the advantages of partnering with the DoD, and providing some funds to fly NASA instruments as part of the SERB / STP process.

Strategic Planning for LWS and STP missions

Background:

The recent NRC Decadal Survey (DS) outlined four high-priority science targets for strategic missions in the STP and LWS lines: 1) Heliospheric Boundary, Particle Acceleration, and Solar Wind Plasma; 2) Lower Atmosphere Driving; 3) Magnetosphere-Ionosphere-Thermosphere Coupling; and 4) Geospace Dynamics Coupling. These are represented in the DS by four notional mission concepts, respectively: 1) IMAP; 2) DYNAMIC; 3) MEDICI; and 4) GDC (listed in the order of implementation suggested by the DS).

Due to budgetary pressures, HPD has followed the highest three recommended DS priorities of 1) completing the current program; 2) implementing the DRIVE initiative; and 3) augmenting the Explorer program, before moving on to implement any of the strategic missions outlined in the DS. The DS prioritization was clear that none of the strategic missions were to be started before the highest three priorities. Unfortunately, in an era of limited budgets, this may mean postponing the strategic missions to the indefinite future, and this would result in the loss of critical science opportunities (e.g. overlap between IMAP and the Voyager mission). The DS also recommended that the STP line be restructured as a moderate-cost, PI-led mission line.

Currently, the next STP AO (STP-5) is scheduled for FY17, but there has of yet been no word as to the process (if any) for any Science Definition Teams or similar efforts. Given the changing budgetary landscape since the DS, as well as the passage of time and developments that may invalidate some of the assumptions in the DS, it may be time to start developing the science targets and associated missions further. There is currently no planned timeline for the follow-on STP mission AOs beyond STP-5, or for the LWS mission AO.

Developing these science targets further, and doing it now, will have several benefits to NASA, allowing it to:

- 1) provide an impetus to start developing political support for these missions, both in the science community and outside. As soon as these missions are perceived to be “real” and there is a path to their implementation, the community will step up and work hard to enable them. Also, there is the potential to build political support for these missions if they start to become a reality.
- 2) determine if there are better ways to achieve the science than the notional mission concepts that were costed in the Decadal Survey, especially if there have been changing circumstances or developments since the Decadal Survey.

Finding:

The GMOWG finds that it would be advantageous to consider beginning NASA-sponsored processes that could develop mission concepts or resource allocations for **all four of the science targets** (IMAP, DYNAMIC, MEDICI, and GDC) further. These processes could include Science Definition Teams (SDTs).

The SDT for the mission to study global dynamics of the I-T system (an LWS science target which is not expected to be PI-led) could be a standard SDT. This SDT would have broad representation, and could result in a single mission concept that could be led by a NASA Center or large external institution, with strong community support and involvement in science and instrumentation.

For the STP science targets, which may be PI-led, there are some nuances. NASA may wish to consider the advantages of funding multiple, smaller SDTs for each science target, and allow the multiple teams to independently submit innovative ways to attack the science target. NASA could potentially provide support from the CATE process to the teams to vet their approach from a cost and risk standpoint. These studies could remain confidential, so as not to prejudice any future competition. NASA HQ could then determine which of these proposed solutions provide sufficient progress on the science target at the most appropriate price point, and set the resources available for the AO based on the SDT reports. Then NASA could consider holding a standard AO process, with the cost and other resources (e.g. launch vehicle) allocated so as to envelope a subset of the SDT reports that provided enough science return at a low enough cost.

In beginning these important studies, it would be important to keep in mind the recommendations of the NRC Decadal Survey. Namely, that the highest priorities include completing the current program, implementing DRIVE, and augmenting the Explorer budget line. The GMOWG finds that there are advantages to starting these SDTs now, but it may be more advantageous for NASA to wait until the higher priority recommendations are fully implemented before implementing these new missions.

Increased flexibility in the upcoming Explorer AO

Background:

Small satellite technology and access to space for small satellites is experiencing rapid development and growth. As this growth continues the risk associated with such technologies will decrease to the point where it can be applied to Explorer-class missions. Satellites as small as 3U are being deployed that address important space science goals. Commercial entities are now successfully assuming the role of finding launches for small satellites as secondary payloads. NASA KSC has issued a Request for Proposals for Venture-class launch services capable of lifting 30 or 60 kg to 425 km orbit. Companies are developing systems such as the “Sherpa” which can serve as “space taxis” to take secondary payloads to orbits different from the primary launch vehicle, including even geosynchronous orbit.

The ability to deploy multiple low cost small satellites allows much greater capability and flexibility to explore spatial and temporal variability in the geospace system. It enables us to view multiple regions simultaneously and provides the ability to study features from multiple geometries. It is anticipated that future Explorer opportunities will result in increasing numbers of proposed missions that take advantage of this technology. There are inherent differences in the risks associated with these new technologies and the risk mitigation strategies that are appropriate to them. For example, the risk approach to satellites constellations is different compared to large, single satellite missions. In the case of constellations, the loss of a single satellite may have small impact on overall mission.

In addition to the growth in small satellite capability, the recent development of the International Space Station as a home for science instruments (AMS, RAIDS, CATS, ISS-CREAM, etc.) is a major new opportunity for Heliophysics. Enabling the ISS to be used as a “facility” for world-class Heliophysics science will provide excellent return on investment and a low-risk way to attack many important and outstanding science problems.

The community is anticipating an AO for a SMEX and Mission of Opportunity in 2016. It is not known what this AO will contain, but the recent Astrophysics SMEX AO had the following cost caps –

SMEX cost cap \$175M (including launch services);

MoO cost cap \$65M;

“Suborbital-class” (balloons/Cubesats/sRLV) MoO cost cap \$35M;

with SMEX missions that want to use NASA-provided launch services being charged \$50M. SMEX missions that use alternative means of access to space can do so, provided they pay for it within the \$175M cost cap and also provide \$2M for NASA launch vehicle monitoring and advisory services.

Institutions across the Heliophysics community are currently formulating mission concepts in order to prepare for the anticipated AO. The current assumption, common amongst community members, is that the Heliophysics AO will be identical to the Astrophysics AO. If the Heliophysics AO were to be significantly different, it would benefit the community to learn about the planned changes as early as possible in the concept formulation process.

Finding:

It would be advantageous for NASA HQ to consider several possible courses of action, all designed to maintain a high level of flexibility in the Explorer program:

- 1) as in the Astrophysics Explorer AO, consider the advantages of permitting ISS investigations to propose up to the full SMEX cost cap. There exist numerous community concepts for large, capable “observational facilities” that could do compelling science from an ISS berth.
- 2) Consider the benefits of permitting Cubesats to propose at the full \$65M MoO cost cap, provided they arrange their own ride, just as the hosted payloads do
- 3) Consider whether it is advantageous to permit Cubesats that fly as secondary rides (rides provided by NASA) to have a larger cost cap, on the order of \$50-65M. There have not been any Class D 7120.5 Cubesat missions flown to date, and these will likely be more expensive than the previous generations of NSF- and HTIDES- funded Cubesats, especially given the lower risk tolerance for Class D missions vs. 7120.8 missions.
- 4) As in the Astrophysics Explorer AO, consider the advantages of permitting SMEX missions to purchase their own ride to orbit, potentially as secondary payloads, provided they stay within the cost cap (notionally \$175M = \$125M plus \$50M for NASA provided launch services)
- 5) Consider the benefits of expanding the largest allowable Cubesat form factor to 12U. Currently, 6U vehicles are under development and will be launching in 2016 (LAICE NSF Cubesat, Dellinger Cubesat, DoD and commercial cubesats). 12U deployers exist and companies, government agencies, and universities are developing mission concepts to utilize this larger form factor.
- 6) HQ may wish to consider releasing a non-binding “Notice of Intent” as soon as possible, outlining the important aspects of the anticipated AO (expected release date, expected cost caps, and whether Category III proposals are likely to be awarded funding – see Technology Development finding in this document).

We find that it would be advantageous for NASA HQ and TMC0 to consider the examining the risk of launching full Explorer missions as secondary payloads or constellations launching as one or more secondary payloads. Can specific criteria be established for mitigating risks associated with such approaches? These risks include the difficulty in specifying the launch vehicle at time of proposal. We suggest NASA explore whether it would be advantageous to undertake some case studies of mission examples and evaluate the risk of implementing those missions with new

launch approaches. What is the source of those risks and what are possible mitigation approaches that the community can consider?

NASA may wish to consider the advantages of implementing a Hosted Payload office to negotiate launches of Explorer and other missions as secondary payloads. NASA could assume responsibility for the overall approach, ensure risk items are addressed, and would work with mission teams early in process. This process could mimic the approach currently handled by NASA for Pegasus launches.

Guest Investigator funding for MMS

Background:

The long-awaited MMS mission was successfully launched on March 12, 2015 to solve one of the long-standing questions in space physics, magnetic reconnection. Understanding the physics underlying magnetic reconnection is fundamental to making progress in understanding heliophysics phenomena, and for distinguishing between competing physical processes. The MMS mission, designed to provide a comprehensive answer to these open questions, will provide much needed high time resolution observations towards understanding the reconnection process. Such understanding will also require theory, modeling, and insightful data analyses of archived data from past missions to complement the MMS observations.

It would help to spur community research using MMS data if there were additional funds available for the community to start analyzing data during the prime mission. Also, the Decadal Survey recommended that a certain portion of each future mission's budget be set aside for guest investigator programs. Although MMS was implemented before this recommendation, it would be a great way to ensure maximum science output of the mission if funds from the STP line could be used to provide enhanced GI funding.

Other near-term missions (Solar Probe, SOC, ICON, and GOLD) would also benefit if LWS or Explorer "future mission" funding could be used to provide GI programs during their prime and early extended mission phases. Or, potentially, there could be advantages to funding these GI programs through the DRIVE initiative.

Finding:

The GMOWG finds that it may be advantageous for the community to have both an open GI (with MMS proposals not allowed) as well as a dedicated call for MMS science proposals in the 2016 ROSES competition. Such a dedicated call would protect science data analysis money for non-MMS proposals during this competition, while helping to kick-start the community's ability to do exciting science with MMS data.

It would be most beneficial if the dedicated MMS call were later in the year, e.g. aiming for a Step 1 deadline in July 2016, to give colleagues who may not be associated with the MMS instrument teams time to familiarize themselves with the available data sets and their specific features, following the expected March 2016 public data release. To increase the science return of the MMS mission, it may be beneficial to have a broadened scope for the GI solicitation, not limited to the official MMS science goals.

In addition, HPD may wish to consider the advantages of setting aside GI program funding out of each mission in the STP, LWS, and Explorer mission lines, even for missions currently in formulation (current missions could be funded through DRIVE). This will help to maximize the science output of those missions, and will provide a way to realize one of the important recommendations of the Decadal Survey.

Developing a framework for sounding rocket mobile campaigns

Background:

We recognize the significantly enhanced scientific return generated by mobile sounding rocket campaigns, which have long been a hallmark of the NASA program. The benefits of the campaigns include access to remote, scientifically interesting locations that cannot be accessed for in-situ measurements otherwise, including possibilities for launch at the location of ground-based instrumentation that can provide critical support for the launches and access to critical regions of the atmosphere where no established ranges exist.

In spite of the critical importance of this capability, there has been no established procedure for developing and reviewing campaign proposals within the framework that NASA has been developed for proposal submissions and review. Currently there is no guidance from NASA Headquarters as to whether and when campaign proposals will be considered and the process that the science community should use for developing such proposals.

Note: Within the community there has been interest in developing proposals for campaigns in Puerto Rico and Svalbard. In spite of planning efforts within the science community and by Wallops, the potential investigators and SRPO at Wallops have been unable to get specific input from NASA Headquarters sufficient to move ahead with either plan. The previous mobile campaign was carried out in 2004. The earliest proposal opportunity will be 2016, which would delay a future campaign to at least 2019, i.e., fifteen years after the previous campaign!

Finding:

NASA may wish to consider the advantages of implementing a proposal framework, as well as a schedule that would allow mobile campaigns to become a regular part of the NASA LCAS program.